The A-Star Puzzle
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Panel discussion section G

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INVITED SPEAKERS: M. Breger, D.W. Kurtz
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Discussion

1. roAp stars

NOELS: How is convection suppressed in model computations?

CUNHA: We do this in two different ways. One is just to make the mixing parameter smaller and smaller. As it approaches zero we are, in effect, suppressing convection. The other way is simply to use the equation for radiative energy transport throughout the envelope. The results are very similar.

REEGEN: I can think of two reasons for the phase lags in the line bisector analysis of roAp stars. Firstly, each bisector refers to a different layer and it is possible that the rotational velocity is different for different layers. The result is a phase lag at the boundaries of each layer. Secondly, the speed of sound is different at the boundaries of the layers and the eigenfrequencies no longer match. Instead, there is a periodic lag in the region where you have a standing wave and this causes the phase lags.

KURTZ: That may be true. What I did not talk about today and what is definitely important in this connection is the effect of horizontal stratification. Oleg Kochukhov is working on this aspect and trying to understand what we see in the line profiles. I think that in some stars the vertical stratification of neodymium is a very good explanation for the phase lags, but that may not be true in other roAp stars.

NOELS: Is it possible that driving in roAp stars may be due to iron? As Georges Michaud has shown, iron can accumulate in certain layers due to diffusion and it may be possible that this could act as a driving mechanism in a certain mass range.

CUNHA: I actually tried this idea, but it does not seem to make much difference for the roAp stars which have high-frequency oscillations, but I did not try the same thing for the low-frequency δ Scuti pulsators.

KURTZ: What do you need to get driving from iron at about 200000 K? I suspect that for the roAp stars the iron layer is too deep to cause much driving of the roAp oscillations because these have high amplitudes only near the surface, so you need a mechanism which operates close to the surface.

SHIBAHASHI: To cause driving the thermal timescale must closely correspond to the pulsational timescale. For the iron ionization zone, the thermal timescale is much longer than the periods observed in the roAp stars.

CUNHA: Yes that is true, although I do not think that by itself is the reason why the iron ionization zone cannot drive high-frequency oscillations. I do not believe that you have to have the same kind of timescale. What you do need is that the thermal timescale in the ionization region must be comparable *or longer* than the pulsation period. You cannot have a shorter thermal timescale, however. The argument that the timescale has to be of the same order as the period is based on the change of the profile of Γ_1, and that is not perfectly symmetric. The argument goes that on one side driving occurs and on the other side there is damping, and that it is basically symmetric. So if the timescale is much larger than the period of the oscillations, the pulsations are almost adiabatic and driving is compensated almost exactly by the damping. I think the real problem is the very small amplitudes when you get down to the region of iron bump, so there would be no way to excite the oscillations.

MICHEL: The destabilizing effect should be active also for the δ Scuti stars. Should we therefore not see short period pulsations similar to those in the roAp stars in the δ Scuti stars?

CUNHA: That is exactly what I was saying, that the thermal timescale has to be of the same order or longer. If it is smaller, then there is no driving. In the δ Scuti stars, whose periods are longer than those of the roAp stars, the thermal timescale of the hydrogen layer (which we believe to be the main driving region) has a smaller timescale than the periods of the δ Scuti stars, but is comparable to that of the periods of roAp stars. That means that we should not see δ Scuti periods in roAp stars due to the H ionization zone.

MICHEL: Yes, I understand that this is true for the periods observed in the δ Scuti stars, but what I was asking is why we do not see roAp pulsations (shorter periods) in the δ Scuti stars?

CUNHA: That is because convection dampens the driving of such oscillations in the δ Scuti stars. Remember that we had to introduce the concept of suppressing convection in the roAp stars by their strong magnetic fields to get driving due to hydrogen ionization. This does not work for the δ Scuti stars.

VAUCLAIR: The iron convective zone is able to destabilize stars in the sdB region (hot end of the horizontal branch), but probably not cooler stars (cf. S. Chapinet's PhD thesis).

SHIBAHASHI: To explain the strange LPV found in the Nd III and the Pr III lines of γ Equ, I am suggesting that those LPV are due to a shock wave generated in the upper atmosphere somewhere between the photosphere and the line forming layer of both Nd III and Pr III. I would like to ask the experts if the possible presence of a shock wave is allowed in the framework of the diffusion hypothesis.

MICHAUD: The question is how much mixing the shocks may cause. In so far as they are, for instance, in a region of vertical magnetic field one can imagine they can be modeled by adding to a one dimensional random walk the effects of gravity and of the radiative acceleration. It would widen the layer created by diffusion but not necessarily eliminate it. So shock waves would not necessarily eliminate the layering caused by atomic diffusion.

MONIER: How do you think we would see evidence of shock waves in the spectrum of an Ap star, and on what timescale?

Shibahashi: So far the evidence is in just the line profile variations of Nd III and Pr III lines during the pulsational cycle. There is a monotonic shift from blue to red, a sudden jump from red to blue, and again a monotonic shift from blue to red. Other lines, such as Hα and Nd II, show more-or-less sinusoidal line profile variations, unlike those of Nd III. We believe Nd III to be formed very high in the atmosphere, which is exactly the region where you may expect to see a shock.

Monier: Why do you not expect that in some cases the shock waves would dampen and completely decrease to zero amplitude?

Shibahashi: We have not yet carried out any computations, so my proposal is just a phenomenological model.

Kochukhov: I think that a comment on the observational aspect of your model is in order. Do we really see a shock in the line profile variability? A shock means a very rapid change in the line profiles as observed, for example, in other types of pulsating stars. I agree that your model potentially explains the blue-to-red moving feature. But currently it seems that your model also predicts a very rapid change in the line profiles and it is not clear if this change is consistent with the observational data. In the observations we see fairly smooth changes of all characteristics of the line profile: line widths, radial velocity, line asymmetry, etc. This aspect needs to be explored in greater detail.

Kolenberg: I am also very interested in knowing what the variations of the moments of the line profile looks like if there is a shock wave. This is something that should be checked to see if it is consistent with the data.

Shibahashi: At this moment, the shock wave model is just an idea which has been proposed in an attempt to explain a very puzzling phenomenon. The details of the theoretical line profile variations should be dependent on some free parameters. So far I have not attempted to see if the computed line profiles match the observations in detail, but I think that the model is promising for explaining the observed line profile variations in some of the roAp stars.

2. δ Scuti/PMS stars

Noels: Are there examples of Pre-Main Sequence (PMS) δ Scuti stars located in exactly the same part of the H-R diagram as the evolved δ Scuti stars? If so, are there any observational differences in the frequency spectra?

Breger: The PMS and the evolved δ Scuti stars are in the same part of the H-R Diagram. For every PMS δ Scuti star you can find at least one evolved analog with a similar temperature and luminosity. Nevertheless, there are still differences in the rotational velocity and the metallicity. These differences may have a larger effect on the pulsations than the difference in evolutionary status. More detailed studies of PMS stars to examine their pulsational behaviour are important.

Zwintz: The first problem is that we do not have spectra of all the pulsators we have detected. We have short runs, of two weeks mostly, or one week, or just a couple of nights, which is not good enough to detect all the frequencies. We just say “here is another point and there is another point”. So we need a longer time series using multi-site observations or perhaps data from space. If we had a frequency spectrum sufficiently dense, we could compare the evolved stars with the young stars; but at this stage this is not possible. There should be differences. Theory predicts differences which originate in the interior of the star. There is probably some observational hint that Herbig Ae stars, which are characterized by emission lines, interact strongly with their environment. So there are some observational indications, but not enough to be convincing.

BALONA: It is extremely difficult to get accurate effective temperatures and luminosities for PMS stars. These stars usually have emission lines, are obscured by dust and are generally too far for Hipparcos parallaxes to be used. So you cannot directly determine their positions in the H-R diagram with any degree of accuracy.

ŠKODA: We have heard that in β Pic there are a large number of modes with high spherical harmonic numbers which appear to be unique to the star. We know that there is a circumstellar disk around beta β Pic. Is it not possible that the line profile variations are not due to pulsation but to periodic obscuration arising from circumstellar matter orbiting the star?

BALONA: In β Pic there is, indeed, nonperiodic obscuration of circumstellar material seen in the H and K lines of Ca II and which is generally attributed to infalling bodies. However, the periodic line profile variations seen in β Pic occur in lines that are formed in the photosphere and are not affected by the circumstellar medium. Therefore, the variations cannot be due to obscuration by the circumstellar medium.

KURTZ: Michaud showed just a few years ago from his models that iron ionization could possibly drive g-modes in Am stars. This has not yet been tested observationally. Are there any observations that can say whether g-modes exist in Am stars?

BREGER: This depends on which of the modes you mean. The observations can exclude relatively short periods around, say, 14 hours. The observational limits become poorer for much longer periods.

DASZYNSKA-DASZKIEWICZ: Breger showed the instability parameter versus frequency. For δ Scuti models we get a peak of instability for the low frequency g-mode range, but not enough to cause driving of pulsations. In this case we can attribute this to our lack of understanding of the interaction of convection and pulsation. So my comment is that the opacities used in the models may still need to be increased.

BREGER: This was an excellent comment, but I must add that the models do not include the effects (if any) of convective blocking.

DASZYNSKA-DASZKIEWICZ: I think that the δ Scuti and the β Cephei stars still have problems with the opacity tables.

BREGER: The groups have very different temperatures, but both show longer-period analogs with gravity modes, as recent discoveries have shown.

DASZYNSKA-DASZKIEWICZ: In theory we should get peaks in both type of stars.

3. The Blazhko effect

Kurtz: Stars showing the Blazhko effect have mostly similar behaviour, but it can range from nearly pure amplitude modulation to nearly pure phase modulation for some C-type RR Lyrae stars. Some of the modulation envelopes are nearly symmetrical such that as the peak amplitude increases, the minimum amplitude decreases. In others, the minimum amplitude changes much less than the maximum amplitude. It is difficult to see how the resonance or magnetic models can match the wide range of behaviour we see in the Blazhko stars.

Kolenberg: Yes, I agree. But both models need to be developed. Also, we cannot exclude the possibility of a combination of both effects.

Breger: In the resonance model, a combination of a radial and a single nonradial mode cannot explain the observed variations. However, what happens when we have four of five close modes? We will probably see amplitude and phase variations in varying degrees. This is seen in δ Scuti stars, for example.

Kolenberg: Yes, it can definitely explained by more than one mode. Observations are under way to detect these modes using high-resolution spectroscopy.

Breger: Although the predicted frequency region with excited pulsation modes may be quite large, a nonradially pulsating star seems to pick several small frequency subregions, in which an especially large number of pulsation modes are excited and observed. To my knowledge, this has not yet been explained theoretically. Could this be related to the resonance model of the Blazhko Effect, which requires many close modes?

Preston: Well I am on my angular momentum kick. I would like to make a comment on Kolenberg's talk. In the case of the magnetic model, the Blazhko periods suggest that the MS progenitors had rotational velocities of many tens of kilometers per second. But to my knowledge there has never been a measurable rotational velocity of a Population II MS star.

Budovičová: How long are the Blazhko periods?

Kolenberg: Typically it is tens to hundreds of days. The shortest Blazhko period is about 11 d and the longest about 500 days. There seems to be no correlation between the period of radial pulsation and the Blazhko period.